Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

SHORTER ARTICLES AND DISCUSSION

THE MODIFICATION OF MENDELIAN INHERITANCE BY EXTERNAL CONDITIONS

A WHOLE number of the *Biological Bulletin* (May, 1910) is devoted to Professor W. L. Tower's article on "The Determination of Dominance and the Modification of Behavior in Alternative (Mendelian) Inheritance by Conditions Surrounding or Incident upon the Germ Cells at Fertilization." The work was done on certain chrysomelid beetles, *Leptinotarsa signaticollis, L. undecimlineata* and *L. diversa,* all occurring in Mexico. Every biologist who takes any interest in questions of heredity will read this exceedingly important paper, and will be profoundly impressed by the data therein presented, but it is possible that he will also be somewhat puzzled. It is in a humble frame of mind that I venture to set forth some of the difficulties, fully realizing that Professor Tower, with his long experience in patiently investigating the chrysomelid beetles, must know what he is about, and that any mere outsider is at a tremendous disadvantage. At the same time, it is after all for the outsider that the paper is written, and it is due to him to make it as clear as possible, and it may be to explain what seem to him insuperable difficulties.

L. signaticollis ♀ was crossed with *diversa* ♂, giving in F_1 about half *signaticollis* and half a blend between the two. The F_1 *signaticollis* mated together gave only *signaticollis,* and further breeding for five generations gave only the same. The blend, however, split in F_2 into *signaticollis,* the blend, and *diversa,* giving the expected Mendelian ratios. (The obvious suggestion here would be that the original male *diversa* was heterozygous, gametically identical with the blend.)

Next, the same cross was again made, using beetles of the same stock but at a reduced temperature. As a result, the F_1 beetles were of one sort only, the blend; and these, when inbred, gave in F_2 a typical Mendelian grouping. These two crosses were repeated eleven times with substantially the same results.

It is to be noted here that *signaticollis* appears to be a form of lower altitudes than *diversa,* and it seems possible that under the reduced temperature the pure *signaticollis* perished, leaving only the heterozygous individuals, giving the stated results. Were this the case there would be no real modification of inheritance. So far good, but now we come to the next group of experiments. Another cross was made, *signaticollis* ♀ × *diversa* ♂, under the same conditions as the first experiment, giving the same results. Then the *same beetles* were used again, at reduced temperatures (as in the second experiment above), and gave in F_1 only *signaticollis,* and this, inbred, continued true for four generations. This experiment was repeated seven times with uniform results. It seems astonishing that the cross in the first set of experiments at a lower temperature should give only the blend, and in the second only *signaticollis.* This, however, is not only explicitly stated, but is illustrated by plates showing the pedigrees of the beetles. In spite of all this there *must* be something wrong, because on page 295 it is stated that the results of the second group of experiments *confirmed* those of the first, and on page 304 it is said that the results of the second were "in every way the duplicate" of those of the first. On page 330 experiment H. 410 (the first group, at the lower temperature) is said to have given beetles exactly like the female parent (*signaticollis*), which subsequently bred true!

Is it possible that the description of H. 410 (p. 294) belonged to something else and was accidentally put in the wrong place when the paper was made up? If so, was the plate then made up from the erroneous description? There is no reason, of course, for doubting the accuracy of the experiments, but certainly there is something wrong with the account of them.

On page 299, in experiment H. 701, there arose a "single pearly white larva [which] gave rise to a female exactly like the parent type and four classes of larvæ." Something has dropped out here, and the statement as it stands is unintelligible.

In experiment H. 700 A (p. 296) a female *undecimlineata* was crossed with a male *signaticollis*, giving *only* the female (*undecimlineata*) type, which bred true for six generations. "The male type was as completely eliminated as if it had never existed." This was not a result of parthenogenesis, as all efforts to induce *undecimlineata* to reproduce parthenogenetically failed. Here it is interesting to note that *undecimlineata* differs principally from *signaticollis* in having fully pigmented bands on the elytra—a positive character. The larva of *undecimlineata,* however, is white, while that of *signaticollis* is bright yellow—the latter apparently the positive character. In the cross, the larvæ and adults were equally of the *undecimlineata* type, although it was shown in another experiment that the larval and adult characters were inherited independently, each of four classes of larvæ giving rise to three classes of adults, with Mendelian numbers. It was found perfectly possible for *undecimlineata* to have yellow larvæ, and for *signaticollis* to have white ones, when the right crosses had been made.

Are we to explain H. 700 A by saying that the result was a genuine hybrid, from the make-up of which was somehow rejected the superficial characters of *signaticollis?*

Another class of experiments, described as "experiments in synthesis" was productive of extremely interesting results. At Cuernavaca, Mexico, a suitable spot was planted with the proper food-plants, and stocked with 210 specimens of *signaticollis,* "from a standard location about a mile and a half distant" and 354 specimens of *undecimlineata.* These freely bred together, giving in the first generation 4,518 *signaticollis,* 11,744 mid-type, and 5,091 *undecimlineata.* Gradually, however, the *signaticollis* increased at the expense of the others, until at length it was the only form present. This looks like natural selection, and it will be noted that it occurs in a region normally inhabited by *signaticollis.* The experiment seems ideally perfect, except for the stated circumstance that there was a standard locality for *signaticollis* about a mile and a half away. From what we know of the migrations of these beetles,[1] it does not seem unlikely that the colony was overrun with immigrating *signaticollis.*

At Paraiso, in the foot hill rain forest, a locality was planted with 100 each of *signaticollis* and *undecimlineata.* This, I take it, is in the *undecimlineata* country. The result was exactly the reverse of the experiment just described—or not exactly that, for the *signaticollis* practically disappeared even in the first filial generation. Many other particulars are given, and there is an important theoretical discussion, but of course Professor Tower has still in hand a great mass of unpublished material of great value. One may hope that its appearance will not be too long delayed.

T. D. A. Cockerell.

---

[1] For example, in six years I had never seen *L. decemlineata* (the potato beetle) in Boulder, Colorado; but this year (1910) it appeared in enormous numbers, flying. The tomato plants in my garden swarmed with them.